MEDICAL MUSEUM.

VOL. III.....No. III.

Observations on the American Alligator. *By* JAMES MEASE, M. D.

ALLIGATORS abound in the rivers and ſwamps of Georgia, and N. and S. Carolina, and thrive equally well in ſalt and freſh water. I ſpent the laſt winter and ſpring, (Dec. to May,) in the iſland of Great St. Simon, on the ſea coaſt of the ſtate firſt mentioned; and having frequent occaſions to go up the river Altamaha, about fourteen miles from its mouth, I muſt have ſeen at leaſt 200 alligators in the courſe of five months, whilſt they were either baſking on the banks of the river, or floating on the ſurface of the water. They were of every intermediate ſize between eight or nine feet, and ſix inches in length. I ſhot ſeveral; one of them was of the length firſt mentioned, and large in proportion.

On the 15th February, a little after ſunriſe, I perceived the animal lying on the banks of a creek, which has lately been cut through the iſland of Little St. Simon's, and apparently in a ſound ſleep, for his eyes were cloſed, and he permitted the barge to approach within about fifteen feet of him; and gave me time to

load both barrels of my fowling piece. His body being covered with a coat of mail, he was ſecure from the impreſſion of even a muſket bullet; but the pliability of the ſkin under the fore paws affords an eaſy entrance to buck-ſhot, which if well directed, ſeldom fail to kill, or at leaſt ſo effectually to ſtun them, as to permit the accompliſhment of their death by the hatchet; and as the extended poſition of the one I am now alluding to, afforded me a fine opportunity of diſcharging my gun into his vulnerable part, I was fortunate enough to fire both barrels with ſucceſs. He was ſo ſtunned, as to permit a rope to be tied round his neck, and being without delay dragged into the water, he was faſtened to the ſtern of the boat. As I was about two miles from home, he had time to recover from his ſhock, and ſoon began to plunge and daſh about with great force, and actually impeded the progreſs of the boat to ſuch a degree, that had it not been well manned, I ſhould have been under the neceſſity of leaving him in the water. Being apprehenſive that he would bite off the rope to which he was faſtened, and thus eſcape, I endeavoured to hook his lower jaw with the boat hook; but he twiſted off the handle above the hook in an inſtant, though more than one inch and a half in diameter. When dragged on ſhore, he was ſtill very active, and opened his ravenous jaws as often as a ſtick was preſented before his mouth, and frequently wheeled round with great celerity to face his adverſaries. On opening his ſtomach, I found a parcel of ſilver fiſh (as they are called) one and two inches long; alſo a block of live oak about a pound weight, the end of which had been burnt, and a ſmaller block of white pine, both of which were worn ſmooth. On mentioning the circumſtance to two gentlemen who were preſent (at St. Simons iſland) they informed me, that various ſubſtances are at times found in the ſtomachs of alligators; that chunks of wood were commonly ſeen, and one of them aſſured me, that he had found a piece of red cloth, and the rim of a braſs ſhoe-buckle. I was prepared to meet with various hard ſub-

ſtances in the ſtomachs of hybernating animals, as I well knew that they are taught by the great Author of Nature, the neceſſity of taking in ſuch materials to ſupport their vitality during their torpid ſtate; but I muſt acknowledge that I was ſurpriſed to find them in the alligator of Georgia, conſidering that they always come out of the water in moderate weather, and eſpecially when the ſun ſhines.

Cateſby ſays, that he found pieces of wood ſeven and eight pounds in weight, in the ſtomachs of ſome of thoſe animals; having the angles ſo worn down that he fancied they muſt have lain there ſeveral months.* Dr. Brickel, who publiſhed in 1743, found in the bellies of two alligators in North Carolina, beſides ſeveral ſnakes, ſome pieces of wood, and a ſtone that weighed about four pounds. B. Romans,† and Mr. Ellicott, in his journal, mention the ſame circumſtance, ſo that there can be no doubt as to the fact. Both the laſt mentioned authors, alſo notice the deep hollow noiſe which the alligators make in coming out of the water in the ſpring, and I was aſſured of the ſame fact by perſons who had heard them at night, in May laſt: but having no buſineſs to call me out on the water after dark, and my zeal not being warm enough to induce me to make a nocturnal excurſion on purpoſe to aſcertain the fact, I can only give it on the authority of others.

With reſpect to the hybernating diſpoſition of the alligator; notwithſtanding Cateſby expreſsly affirms that the animal in the ſouthern ſtates remains torpid, from the month of October till March, in caverns or hollows in the banks of rivers, I muſt declare, that with the exception of two or three times, I did not go up or come down the Altamaha, from December to May laſt, without ſeeing thoſe animals; and I may add, that I never

* Natural Hiſtory of Carolina, Vol. II. p. 63.

† Account of Eaſt and Weſt Florida, New York, 1775.

heard of their hybernating diſpoſition from any one in Georgia; and conclude, as well from the ſilence of the inhabitants on the ſubject, as from my own obſervation, that the aſſertion is not correct.

Alligators frequently commit depredations on poultry; but dogs are ſaid to be their favourite food: and an occaſional amuſement with the inhabitants on the ſea board, when on the water, is to cauſe a dog to howl by pinching his ear, in order to cauſe their enemies to exhibit their creſted heads above the ſurface of the water round the boat.

The tenacity of life in the alligator, if life it may be called, is very great, and in this reſpect, fully equals the tortoiſe. One hour and a half after the head of the one I mentioned before to have killed, had been ſeparated from his trunk, I preſented a ſtick to the mouth, when it opened half its ordinary width: the irritation of the mouth was repeated ſeveral times with a ſimilar reſult.

I heard of no injury ſuſtained by any perſon from their bites; probably becauſe people do not expoſe themſelves to their attacks in the water; but I can anſwer for their ſhyneſs when on ſhore; for in general it required ſome adroitneſs to obtain a ſhot at them, as they conſtantly ſlid down the banks into the water, when they perceived the approach of a boat. In other countries,* however, they are ſaid to attack perſons bathing in rivers.

They emit a very ſtrong and diſagreeable muſky ſmell; but this quality forms no objection to them as an article of food with the newly imported Africans; for when I had the head boiled on the bank, in order to preſerve its ſkeleton, ſome new

* In Manilla. *See Navarette's Travels.*

negroes who had but lately arrived from Charleſton, eagerly devoured the fleſh as it was cut off, and actually contended for it. The tail, however, as I was told by ſeveral perſons, is free from the muſky ſmell, and I know ſome who eſteem it a delicacy little inferior to turtle, when cooked in the ſame way as that rich ſhell-fiſh ! !

Their teeth are remarkably white, and conſtitute a favourite necklace with the blacks, for their children, on the ſuppoſition that they eaſe the proceſs of dentition. I could not but ſmile at the weakneſs which leads them to the uſe of the ſilly charm; but when I recollected how many among the whites, poſſeſſing the advantages of greater light and information, religiouſly believe in the power of the Job's-tears necklace in the ſame caſe, and alſo in that of the blood ſtone (hæmatites) to check or prevent bleeding from the noſe or lungs; my idea of the negro weakneſs was conſiderably leſſened.*

The alligator lays its eggs in the woods, or in marſhes near the rivers, and leaves them to chance. A few days before I left the coaſt of Georgia, (in May,) two young ones were found in a wood near a creek, and brought to me. They were apparently not long before hatched: their bodies were about four inches long, and their tails about five inches. When I preſented a ſtick to their jaws they opened them, and exhibited teeth already prominent. I endeavoured to preſerve them alive during my voyage, but the rolling of the veſſel occaſioned their death.

* To the inſtances above mentioned of Chriſtian weakneſs, might be added, that of the belief in the powers of the ſnake ſtone, to extract the poiſon of venemous or rabid animals, by applying it to wounds made by them, a belief which is univerſal in ſome parts of Virginia. More on this ſubject, however, hereafter.

On the Adulteration of Britiſh Malt Liquors. By JAMES MEASE, M. D.

THE very excellent effects we often derive from the uſe of porter as a remedy in diſeaſes, renders the preſervation of its purity an affair of very great importance. Hitherto, when occaſion required our preſcribing porter as a drink, it has generally been the practice to recommend that article of Britiſh manufacture, from a deep-rooted prejudice in both phyſician and patient, that it was ſuperior to our own in the important points of ſtrength and flavour. There have been, however, ſome among us, who being devoid of prejudice, were candid enough to acknowledge, that the Philadelphia porter had greatly improved within the laſt fifteen years, and have contented themſelves with uſing our home-made ſtuff. Some amuſing experiments too, which were occaſionally practiſed upon the "knowing ones" have ſerved to convince us that the power of diſtinguiſhing between the domeſtic and imported article was not ſo eaſy as they thought.*

Some men of taſte among us, had even imagined, that they perceived within a few years an evident diminution in the quality of Engliſh porter, and that inſtead of being a lively, invigorating cordial as formerly, it was thick, laid heavy on the ſtomach, of an unpleaſant taſte, and never failed to confuſe the

* The ſame prejudice prevailed with reſpect to Cheeſe, and the praiſes of the Glouceſter and Cheſhire manufactories have reſounded all over the world: the only reaſon for which was that the Italian and Dutch cheeſes were not made in quantities ſufficient to compete with the Engliſh in foreign markets. The offer of a premium from the Agricultural Society of Philadelphia in 1790, which was obtained by Mr. Matthewſon of Rhode Iſland, ſoon taught us that as good cheeſe could be made in the United States as in any part of the world. At preſent the manufacture of Capner and Choyce of Flemington, and of Exton of Pittstown, New Jerſey, is *much ſuperior to any imported, whether Britiſh, Italian, or Dutch.*

head upon taking an extra-drink. Unwilling however to truſt their own judgment, they were diſpoſed in ſome meaſure to aſcribe the alleged alteration, to their being ſo much accuſtomed to our own weak liquor, as not to reliſh the powerful ſtuff of the Britiſh breweries. I know one gentleman, indeed, who poſitively aſſerted the manifeſt falling off, of foreign porter, and from being a conſtant cuſtomer, gave it up inſtantly. The truth upon this ſubject is now divulged. We all know, that the exciſe upon hops and malt, are permanent ſources of revenue in Britain; and that it has been gradually riſing from year to year until the preſent day; it appears alſo from the agricultural reports of the different counties in England, which were drawn up by order of the national board of agriculture, that the hop culture is extremely uncertain, and that for ſome years paſt, the produce has not been nearly equal to the demand: from the combined effects, therefore, of high firſt coſt of the hops, and exceſſive duties upon them and malt, the *brewers* have *ceaſed to employ either;* but as porter and beer are univerſal drinks in England, ſome ſubſtitutes were neceſſary, and it appears from a publication in an excellent work entitled "The Agricultural and Commercial Magazine," now going on in London, that a plant known by the ſeveral names of May-weed, Morgan, Gooſe-weed, has been long ſubſtituted to great advantage for the hop, and that a man in Derbyſhire acquired an independency by excluſively cultivating it for the purpoſe ſtated. See the above work, Vol. III. p. 14. But of late more injurious ſubſtitutes have been employed, which ſufficiently account for the evident falling off of Britiſh malt liquors. In "Malcolm's Huſbandry" lately publiſhed, after acknowledging that "beer, ale, and porter, have received greater deterioration ſince the ſeaſons of 1800 and 1801, than they had experienced from the time of Henry I. to that period," it is ſaid, "the reaſon is, that neither HOPS nor MALT are now uſed in the manufacture. The ingredients of the former, he ſtates to be "grains of Paradiſe, Capſicum, black pepper, Cori-

ander ſeed, *Coculus indicus*, (an intoxicating drug) Quaſſia, *Liquorice, brown ſugar*, SHAG TOBACCO, *Sal martis* (or green vitriol): and among the materials of the latter, he enumerates the following; *Coculus Indicus*, liquorice or Spaniſh juice, treacle, SHAG TOBACCO, alum, green vitriol, iſinglaſs and quaſſia.

Mr. Malcolm is land-ſurveyor to the Prince of Wales, and the royal Dukes of York and Clarence: his aſſertion therefore muſt be taken as true, for it is not to be ſuppoſed that a man of his reſpectability would riſque the expoſure of ſuch an impoſition, unleſs he was well aſſured of the grounds upon which he made it.

From Barrow's intereſting travels in Africa, it appears, that "Aloes has of late years been much employed in the porter breweries of London, which has occaſioned an increaſed demand for the article.*

But if any confirmation be neceſſary of the aſſertion reſpecting the diminiſhed ſtrength and adulteration of Britiſh porter, it may be obtained by a reference to the "*Commercial Report*," regularly publiſhed in Philip's Monthly London Magazine, which is ſaid to be conducted under the direction of the juſtly celebrated Dr. Aikin. After giving a melancholy account of the dearneſs of coal, meal, and other common neceſſaries of life, the writer adds, "Porter when of proper ſtrength and age, ſo excellent a beverage for the labourer, is by the impoſts levied on it and on malt, now ſo poor a ſtuff, that a working man thinks it neceſſary to ſwallow immediately after his

* The *aloe perfoliata*, from the juice of which, by inſpiſſation, the well known drug Aloes is prepared, is a ſpontaneous production of ſouthern Africa, where large tracts of ground, many miles in extent, are covered with it. The quantity of inſpiſſated juice, exported from Cape town in four years, (1799 to 1802,) amounted to 341,927 lbs. value 25,665,4 rix dollars. *Barrow's Travels*, p. 305.

beer, a glaſs of poiſonous gin, to hinder the former from doing him harm.*

The neceſſity for ſuch an obſervation at that time is the more remarkable, becauſe it appears from another report that the crops of hops in 1803—4, were the two largeſt ſucceeding each other ever known.† The crop of 1805, was as bad as that of the two preceding years was good; blaſt, fly, lice having aſſailed it.‡ "The crop," ſays the writer, "is found not to be equal to a quarter of a year's conſumption; the deficiency muſt therefore be ſupplied by the ſtock on hand of the growths of 1803—4, and it is calculated, that after this year's (1806) conſumption, there will not be more than half a year's conſumption left of all ſorts to meet the next year's crop.§"

From the above extracts, we find that even in the moſt abundant years, the demand for hops is either greater than the poſſible ſupply, or that in order avoid the enormous duty on the article, malt liquors, (ſo called,) are manufactured without it, and others ſubſtituted, ſome of which poſſeſs highly injurious qualities, which ſhould induce us entirely to prohibit the uſe of Britiſh porter, or ale, to our patients. This we may do with more advantage, becauſe the manufacture of thoſe liquors in Philadelphia, has arrived to as great a degree of perfection as is requiſite either for the purpoſes of health or gratification.

* Monthly Magazine, January 1804.

† Monthly Magazine, March 1805. The produce in 1803, amounted to 199,205 bags; and in 1804, to 177,617. The duty upon the laſt crop was £.386,124,19.5½. In 1802, hops ſold for £.16 to £.20 per bag!

‡ Monthly Magazine, Auguſt, September and October, 1805.

§ Monthly Magazine, March 1806.

Account of the Efficacy of the external Application of the Geranium Maculatum, in ſtopping Hemorrhage. *By* JAMES MEASE, M. D.

THE following are the particulars of the caſe of hemorrhage, cured by the application of crow-foot or geranium maculatum, which occurred under the notice of Paul Cooper, of Woodbury, New Jerſey, and mentioned in my paper, in one of the numbers of your Muſeum.

The ſon of Mr. David Cooper, near Woodbury, partially divided the artery at the wriſt with the point of a hatchet in trimming a tree; the wound bled profuſely, and an aneuriſmatic tumour of the ſize of a pullet's egg, was quickly formed. Dr. Hendry, who was immediately called, applied the tourniquet, and alſo a piece of flat lead to the tumour; and apprehending that the uſual operation would be neceſſary, requeſted the aſſiſtance of Dr. Wm. Shippen from Philadelphia. On the arrival of that gentleman, the operation was reſolved on; when the father of the young man inſiſted upon the trial of a vegetable remedy, which he ſaid he had learned the uſe of from one of the aborigines of our country. He immediately repaired to the woods, and returned with ſome of the ſpecific, which was pounded in a mortar with a little cold water and applied to the part, and in a ſhort time, to the great ſatisfaction of the ſufferer and his friends, immediately checked the bleeding. The tourniquet was left on as a precautionary meaſure, but fortunately no occaſion offered for uſing it. In the courſe of a few days the wound healed, and the young man had no further trouble.

Dr. Hendry, from whom I obtained the above particulars, alſo verbally communicated to me the following caſes.

1. A man in pruning a tree divided the ſtout muſcles of the fore-arm in an oblique direction: the wound was full four inches

in length, and bled profuſely from a large artery, and numerous ſmaller veſſels. His ſhirt ſleeve was filled with blood, for being made tight round his wriſt and fore-arm, it prevented the blood from eſcaping, and forming a coagulum round the bleeding orifice, checked for a ſhort time a further effuſion.

The powerful effects produced by the *geranium* in the former caſe, induced Dr. Hendry to apply it in the preſent: accordingly he procured ſome of the roots, and after waſhing and pounding them, filled the wound therewith: the effect upon the ſmaller veſſels was almoſt inſtantaneous in checking the profuſion of their contents; and the bleeding in a ſhort time entirely ſtopped; and although as in the former caſe, the tourniquet was very properly permitted to remain, yet no occaſion offered for uſing it.

2. Another caſe occurred of a wound in the ankle from a ſcythe, which had bled ſo profuſely as to cauſe the man to faint; but on the application of the geranium by Dr. Hendry as above, it ceaſed in a ſhort time.

3. In the inſtance of a violent vomiting of blood, which had reſiſted a variety of remedies, an infuſion of the plant in water, produced the deſired effect in a few minutes.

4. Another inſtance mentioned to me by Dr. H. of the aſtringent effects of the geranium, was that of a young man who had a moſt obſtinate hemorrhage from the ſocket of a jaw tooth which had been extracted. An attempt was made by a phyſician from Philadelphia, to cloſe the bleeding orifice by burning it with a red hot needle, but without effect; on the application however, of the geranium, the bleeding ſoon ceaſed. In conſequence of the virtues of the geranium having been ſo often experienced about Woodbury in caſes of hemorrhage, the inhabitants have been induced to cultivate the plant in their gardens; and it

would be well if their example were followed by every one in the country, for though providence has diffuſed the valuable plant over every part of our country, yet as it grows principally in the woods, and the accident it is intended to relieve may admit of no delay, and often happens in winter when the plant cannot be found, it ſhould be transferred to every garden, that it may be at hand when wanted.

Caſe of Paralyſis, apparently cured by Mercury and Bliſters. By E. Griffiths, M. D.

JUNE 30th, 1806, I viſited C. B. a boy aged ſeven years. On the 27th inſtant, he complained of a ſtiffneſs and inability to ſtraighten his fingers. In the morning of the 28th, his mother noticed him and his brother playing in bed, to all appearance perfectly well; but on taking him up he fell on the floor; ſhe ſet him on his feet again, when he fell a ſecond time. It was now obſerved, that he had loſt all muſcular ſtrength in his lower extremities; the whole ſpine from his head downward was ſo weak that he could not ſit upright or ſupport his head. He retained the power of voluntary motion in his ſhoulders and elbows; his hands were quite uſeleſs, with the fingers immoveably half clenched. His reſpiration, deglutition and ſpeech were unimpaired; ſenſes and mind natural and healthy; no fault in the ſphincter ani; he paſſes water very ſeldom, but feels no inconvenience from the long retention, and it is in rather ſmall quantity. His tongue is white, otherwiſe he has no ſymptom of indiſpoſition about him, except the above mentioned.

I examined the ſpine accurately, but could diſcover no fault in it; the family had not heard any complaint whatever; from

all which, as usual, worms were blamed. I gave him valerian and calomel freely; it purged him, but brought away no worms.

July 1st. Symptoms as yesterday. ℞. pulv. sal. nitri, grs. x. tartar emetic gr. $\frac{1}{6}$ every two hours.

2d. Symptoms as yesterday; continue medicine, and apply blisters to the ankles. A slight eruption begins to appear on the forehead.

On the 3d and 4th, he complains of some strangury from the blisters. The medicine is continued with the addition of cal. gr. 1 to each dose.

The 5th. Blisters to the wrists and continue medicine.

On the 8th, I observed his gums a little swelled. The eruption has spread all over him except the thighs and legs; itches very much and looks very red. He has much more command of the voluntary motions, and is stronger than heretofore, except the feet and ankles which he has no power over, but they are perfectly sensible to any injury offered them.

9th. His pulse is more than usually active, with a slight yellowness of the tongue. Continue medicine, and apply muriate of soda to his feet.

17th. His medicine has been continued without producing a salivation. Many of the eruptions have suppurated and are drying. He can move his feet and toes a little, and begins to walk with the help of some person to support him under the arms. The family now informed me that the patient, when well, was very fond of imitating tumblers, and always trying to stand on his head. I prescribed a large blister to the nape of the neck, and omitted the medicine.

On the 18th, he was able to raiſe himſelf on his ſeat in bed without help; the 19th, he ſtood alone and walked a little, ſteadying himſelf by holding his father's hand.

Another bliſter was applied on the lumbar region on the 26th. His amendment has been very rapid ſince. He is able to walk very well about the houſe alone; he rode in a carriage five miles out of town and back, on the afternoon of Auguſt 1ſt.

He was now ſent to the country, where he was recovering his ſtrength rapidly, as I was informed ſoon afterwards.

Caſe of abdominal Swelling, cured by the Application of Nicotiana, or common Tobacco. Tranſlated from the Italian, and communicated to the Editor, by E. CUTBUSH, M. D.

A YOUNG woman, aged 23 years, of a temperament inclining to the ſanguineous, relates, that in the early periods of her infancy, a ſwelling was obſervable in the vicinity of the epigaſtric region, which prevented her parents from binding her with the *ſwathe-band*, at that time in uſe. As ſhe approached the age of puberty, the tumor increaſed, and ſhe menſtruated with great *uterine* pain; it then diſappeared for ſome months, and produced flattering appearances of the entire removal of the diſeaſe. Her menſtrual diſcharges were not regular, and were ſmall in quantity; preceded, and accompanied, by the moſt terrible ſpaſmodic contractions; after the diſcharge of a ſmall quantity of mal-conditioned blood, the vagina ſuffered very much by an acrid humour, which corroded its delicate membrane, and which has continued, more or leſs, to the preſent period of her age.

The tumour, in the above deſcribed part, appeared again of greater ſize, but did not ſpread, in the courſe of eight years, except on certain occaſions, when the *nervous contractions* affected the internal parts of the abdomen; that in ſuch circumſtances, the patient complained of a ſenſe of *ſtretching*, in the functions of the diaphragm, and of ſuffocation. The tumour was, therefore, declared by ſome to be the effect of *hyſteric cramp;* in fact, the ills which tormented this unhappy damſel, were truly Proteiform. She ſometimes complained of general pain in her joints, as though they had been ſuddenly broken, at other times, of a diffuſed ſwelling of her abdomen. She was, at length, attacked with fever and maraſmus, accompanied by eruptions reſembling ſyphilis: to this ſucceeded another phenomenon, which is worthy of remark; a ſuppuration commenced around the nails of her fingers, one after the other.

Laſtly, when in the 21ſt year of her age, the diſeaſe unfolded itſelf with general *convulſions*, in all the voluntary motions, producing at the ſame time, clonic and tonic convulſions: the abdomen was diſtended ſimilar to tympanites, a convulſive dry cough, frequently threatened ſuffocation. She ſuffered very much by a total loſs of appetite; ſmall quantities of nutriment, ſolid or fluid, irritated her ſtomach, and were *frequently* diſcharged as ſoon as they were taken, in an unchanged ſtate; even ſimple water was not retained. Frequently in the day when attacked with convulſions, aphonia and blindneſs, were common ſymptoms, but her hearing was exquiſitely ſenſible. She complained of a general tremor, which was moſt troubleſome at night; the convulſions were more or leſs violent, one hour before, and after ſunriſe.

Before, and at the period of her difficult menſtruation, ſhe complained of moſt "cruel ſenſations" in the vicinity of the uterus, and during the ſanguineous diſcharge, the ſpaſmodic pains increaſed, and were moſt violent, when the general con-

vulſions ceaſed. The *nervous tympanites* in this ſtage, was more painful and hard. The continuance of this tragic ſcene, impaired all the functions of the animal economy, and obliged her to keep her bed. The diſeaſe baffled the moſt efficacious means that the medical art could afford; opium was at length given, to allay the convulſive tremors, in the daily uſe of which, ſhe paſſed eight months: in the ſpring of 1805, her frame appeared to have acquired vigour, ſhe left her bed daily, and at length, was enabled to walk. The *tympanitic tumour*, or rather the *hyſteric cramp*, maintained its former appearance, and obliged her to keep her bed, before and after menſtruation.

At this period of her diſeaſe, ſhe conſulted Edward Cutbuſh, M. D. ſenior phyſician of the American marine hoſpital at this place, who ordered the freſh leaves of nicotiana, boiled in vinegar, to be applied to the abdominal ſwelling, by which the tumour was *totally* diſperſed, and the painful contractions within the abdomen removed; no part of the diſeaſe remains, (except a ſlight ſyncope, which daily affects her towards evening,) a troubleſome watchfulneſs, want of appetite, and a looſeneſs in her bowels.

Syracuſe, July 1805.

The above is an abſtract of a circular letter, directed to different parts of Sicily.

SIR,

IN conſequence of the earneſt ſolicitation of the parents of the young woman, whoſe caſe is above ſtated, by one of her phyſicians, ſhe was brought to my houſe in Syracuſe, to be examined. I received from herſelf and parents, a hiſtory of her caſe, which correſponded very nearly with the above ſtatement. Her parents informed me, that they had conſulted 33 phyſicians and ſurgeons of Naples, and different parts of Sicily,

without receiving any advantage; ſome were of opinion, that the ſwelling was owing to a collection of water in the uterus; others, in the ovaria; others, that it was an enlarged liver; finally, two or three were ſtrongly impreſſed with the idea, that it was an *extra-uterine fœtus*, which produced all the diſtreſſing ſymptoms above ſtated.

On examination, I found a very large ſwelling extending from the epigaſtrium, in a diagonal direction, to the anterior ſpinous proceſs of the right ilium. The tumour had a number of inequalities on its ſurface; no fluctuation could be felt; ſhe could not bear it preſſed without ſuffering great pain. I muſt confeſs, I did not give any deciſive opinion on the caſe, it being perfectly new to me, and eſpecially, after the numerous contradictory opinions and practice of the firſt phyſicians of Naples and Sicily had failed in giving relief; ſhe had been twice under the liberal uſe of mercury in Naples and Syracuſe; in the latter place, by direction of a ſurgeon belonging to lord Nelſon's ſquadron, when his lordſhip was there in 1798, without any beneficial effect. From the hiſtory and examination, I entertained no hope of relieving her; but the ſolemn entreaties of her parents determined me to make trial of a remedy, which I had found uſeful in diſcuſſing obſtinate tumours, and which finally terminated a diſeaſe, that had been the ſource of great diſtreſs to the unfortunate female, and which, doubtleſs, proved the diſeaſe to have been an hydropic affection of the uterus, or right Fallopian tube, though no undulation could be diſcovered. I directed the leaves of the nicotiana, recently collected, to be ſtewed in vinegar, and applied to the abdominal ſwelling. The firſt application produced, nauſea, vomiting, vertigo, great depreſſion of muſcular ſtrength, copious perſpiration, and a looſe ſtate of the bowels; her pulſe became very ſlow. In conſequence of the violence of the above ſymptoms, the application was not long continued;—but on the ſucceeding day, it was repeated, morning and evening, and produced

all the above ſymptoms, but in a leſs degree, attended with an immenſe flow of water from the *vagina*. The application was continued twice a-day, for one week, when its effects on the ſyſtem were leſs powerful, but I was informed with the moſt rapturous expreſſions, that the tumour had diminiſhed very much. The day following a prieſt was diſpatched to inform me, that the water was continually running from her as ſhe walked her room. The remedy was continued about 20 days, but the ſwelling diſappeared entirely before the fourteenth. No medicine was given, except, occaſionally, a ſmall quantity of opium or wine during the day.

When the application of the tobacco was omitted, her abdomen was perfectly ſoft; no induration could be felt in any part, and ſhe could bear it preſſed without pain. She was occaſionally attacked with ſyncope, and complained of want of appetite and ſleep. I adviſed a bandage to be applied around her body, a courſe of tonic remedies, a generous diet to be gradually increaſed, equitation, and cheerful company.

I ſaw her in Octobr 1805, ſhe informed me that all the *functions* of her body were natural; her countenance was florid and cheerful.

April 1ſt, 1806, I was informed that ſhe remained in good health. It is difficult to account for the modus operandi of tobacco in this caſe, unleſs the violent commotion, which it excited in the ſyſtem, ruptured the cyſt, which probably contained the water.

I conceive the external application of tobacco, as a remedy in many diſeaſes, demands more attention from phyſicians, than it has generally received. In obſtinate conſtipation of the bowels I have applied tobacco, ſtewed in vinegar or water, to the abdomen, with the greateſt ſucceſs; even after powerful cathar-

tics, enemata of different kinds, injections of tobacco-ſmoke or the infuſion of the plant have failed, and conceive it preferable in many caſes of aſcites, to the common mode of adminiſtering it internally, in the form of tincture or infuſion.

I am, ſir,

With eſteem, Your's,

EDWARD CUTBUSH.

Philadelphia, October 14th, 1806.

JOHN REDMAN COXE, M. D.

Review of a Work, entitled "Relacion de la Calentura bilioſa remitente amarilla, que ſe manifeſtò en Filadelfia, en el año de 1793. *Por el Doctor* BENJAMIN RUSH, *Profeſſor de Inſtitutiones y de Medicini Clinica en la Univerſidad de Penſilvanìa. Traducida de la ſegunda edicion publicada en Filadelfia, en* 1794, *&c. De order ſuperior. Madrid, en la imprenta real año de* 1804." *or, "An account of the bilious remitting Yellow Fever, as it appeared in the city of Philadelphia, in the year* 1793. *By* BENJAMIN RUSH, *M. D. profeſſor of the Inſtitutes and of Clinical Medicine in the Univerſity of Pennſylvania." Tranſlated from the ſecond Edition, publiſhed in Philadelphia,* 1794, *by authority. Madrid, Royal Printing office,* 1804. *By* FELIX PASCALIS, M. D. *of New York.*

IN the review of this work we will advert, firſt, to the preface, ſecondly, to the catalogue of all the European and American publications on the yellow fever; thirdly, to the critical

notes; fourthly and laſtly, to a few diſſertations added and introduced for the illuſtration of the original text.

It would require ſome explanation to account for what motives the author's name is withheld from a work, which, for national concerns and by authority, is deviſed and extenſively circulated. It is not unlikely, according to Spaniſh etiquette, that no name was required, while its officiality could not be attributed but to a certain academical rank in the medical faculty. Of that circumſtance, however, we are not poſitively informed; we onlycan affirm that in Spain, the whole work is avowedly attributed to *Sig. Dr. Don Ignacio Ruiz de Luzuriaga*, ſecretary of the Royal Academy of Medicine, of Madrid.

The claims of this gentleman to a diſtinguiſhed reputation for medical and literary eminence, were at firſt eſtabliſhed on his extenſive education purſued in the principal univerſities of Europe, under the ſpecial order and patronage of the king, and honoured in Edinburgh, Paris, and Montpellier, with the uſual academic degrees and adoptions. What other brilliant titles have been acceſſary to it, beyond the merit of ſuch labour and conſiderable learning diſplayed in this production, we do not know. We will only remark, that Luzuriaga having put himſelf at the head of the importers and contagioniſts in Spain, has not entered into a luxuriant and fertile ſoil for philoſophical minds to explore. *Obſervations and facts* are always dubious and perplexing; in the innumerable range of thoſe that can be adduced in controverſy, they muſt loſe their authoritative tendency, and ultimately remain unnoticed. Undefined notions of ſpecific poiſons in the air, unchangeable as the elementary principles of the atmoſphere, are aſſumed myſteries, totally unconnected with the known laws of nature; the exiſtence, importation, reproduction and different habitudes muſt conſtitute another ſcience, with which the whole world is ſtill unacquaint-

ed. Upon what ground or baſis ſhall then this ſcience repoſe; and with what deſcription of philoſophers ſhall the contagioniſts and importers aſſociate themſelves? It cannot be defined. It is to be feared, therefore, that however qualified is our Spaniſh writer to rank high in Medical Philoſophy; his fate is now contrarily decided; he is doomed to an unpropitious ſyſtem for progreſſive inſtruction and diſcovery, and in the ſame proportion, that every day enlightened men of all nations ſteadily advance in the moſt unknown paths, and receſſes in nature, he muſt the more recede from them, be diſtanced and ingloriouſly remain an uſeleſs writer, if not a troubleſome one.

1. The preface of this tranſlation offers, at firſt, an uſeful hiſtorical compendium of the yellow fever. With pleaſure we find in it, ſatisfactory proofs that it had anciently exiſted in Greece and the Archipelago, although in a form of more moderate ſymptoms; that two hundred years after the diſcovery of America, it was obſerved at different periods, in many ports and iſlands of this new world. The ancient belief of the French tracing that peſtilential diſeaſe to their fleet from Siam, is alſo judiciouſly refuted by that only fact, that if imported from that diſtant and hot region, it had not invariably proved more fatal to ſtrangers and Europeans, than to the natives or creoles of Weſt India iſlands and tropical countries.

Let us now ſtate what are the opinions and doctrine of Dr. Luzuriaga, reſpecting the origin of the yellow peſtilence. It muſt be taken for granted, that hoſpital fevers, gaol, camp, and ſhip fevers, are all contagious, and ſome of theſe produced the primitive fomes, which with the action of heat, moiſture, and exhalation, conſtituted the degenerated poiſon of the yellow fever. This aſſertion requires but a few hiſtorical facts, to be vindicated, of veſſels for inſtance, and crews, ſoldiers, and hoſpitals in America, which experienced anciently malignant fe-

vers with great mortality, &c. Thus was the fomes of yellow fever originally produced.

Theſe firſt outlines might, however, ſuggeſt a coincidence with the famous declaration of the Academy of Medicine of Philadelphia, (members all named) which ſtated the yellow fever to be a higher degree of the bilious remitting of hot climates; this doctrine was conſtrued to invalidate any idea of ſpecific contagion, it is of courſe carefully removed by a competent number of written teſtimonies in proof of the eſſential difference of the one from the other.

With the proviſion of all theſe premiſes, our writer proceeds, and argues apparently with much reaſon and conſiſtency, to evince the truth of his favourite theory. Graſping at all the names and noſological definitions which this diſeaſe has received, he finds them all incorrect or unmeaning, until he falls upon the *peſtilential* character. Here he dwells with ſatisfaction, becauſe the "true plague" as he calls it, is the only medium of compariſon to which he can refer the evidence of all our writers, reſpecting certain peſtilential (of the true plague) ſymptoms which we have noticed in our epidemics. Now, he goes farther; all the negative characters of the Yellow Fever we had obſerved; that of being ſpontaneouſly checked or irregularly diffuſed; to break out far and wide, all at once, to ſpare certain deſcriptions of people, and to ſweep all the others; forſooth, they are all genuine characters of the *true plague*, innumerable documents are at hand to prove them to be ſo. Moreover, as well as E. Smith, of New York, could deſcribe the plague of Athens, Lucretius has repreſented that of the United States. Juſt as the inhabitants of our cities, eſcaping from the horrid manſions of contagion and death, could not infect the adjacent countries, the ſame happened in Greece when 4,000 reinforcements ſent by Athens to Potidea never could infect it, although they periſhed 1050, themſelves. Plague and Yellow

Fever, are therefore the ſame diſeaſe, with the difference only of a few ſymptoms. Both are fatal and CONTAGIOUS; both require the ſame ſyſtem, the ſame precautions and ſevere meaſures, to be extirpated from the dominions of his Catholic Majeſty!!!

We muſt confeſs, were it ſo well granted and aſcertained, that the Aſiatic plague is contagious, our controverſiſt would ſtand victorious, yet a while.

"Owing" ſays he "to the many errors which have exiſted, and were conveyed by improper names affixed to the American peſtilence, a ſufficient degree of fear being removed altogether with neceſſary reſtrictions, it was introduced in Malaga in the year 1741, in Cadiz, in 1731—36, and afterwards, in '44 and '46. In this town it was raging alſo during the ſummer and fall of 1764."

Dr. Luzuriaga finds no objection, in the above nor in any other recurrences, to admit the moſt remarkable and ſickly atmoſpheric circumſtances, of heat, moiſture and local exhalations, which, in his theory, are originally and eventually acceſſory, to the formation and developement of the contagious poiſon, as they have the power to extricate it from other fevers, to expand it, and ultimately give it the peſtilential character.

In the intereſting account of the introduction of the Yellow Fever into Spain, during the ſummer of 1800, by the ſhip *Dolphin*, from Havanna and Charleſton; our writer has thus prepared himſelf to meet with, and to overcome every difficulty. An exceſſive heat with unſalubrious winds, after abundant rains, were forming an atmoſpheric conſtitution, than which, being combined with the exotic fomes, nothing could be more fatal. Being however ſuſpicious, that many of the *obſervations* and *facts* adduced to prove the actual importation, would not

bear a ſevere ſcrutiny, he takes the wiſe method of ſecuring a great latitude to all the poſſible ways by which it might have been introduced. Many other veſſels indeed, from Vera Cruz and different ports of the United States, anchored in Cadiz, ſome time after the arrival of the *Dolphin*, and he declines to decide which of them was the immediate cauſe of the calamity. He now maſterly deſcribes its progreſs and continuance throughout the ſouthern provinces of the Peninſula. A difficulty is in the way. How Cadiz, the fineſt, the cleaneſt, and healthieſt ſpot, could ſuffer ſuch a ſevere intenſity of the peſtilence and proportion of mortality? Why, after her emigrants had infected all the adjacent neighbouring towns, Cadiz remained, as it were, on a central point, opened and expoſed to the reach of their combined effluvia, and to their current of peſtilential influence.

When relating the ſucceeding occurrences of Yellow Fever in 1801 and 1803, we are ſorry to find our medical obſerver inconſiſtent with the principal tenets of his theory, for he adverts no more to any mode of *tranſmiſſion* or *formation* of the American plague, although it was equally contagious, eſpecially in Medina Sidonia, a large inland and elevated town; certainly, the poiſon of former years could not have been reproduced after its total extinction, unleſs it was of *home-engendered nature*. It was, perhaps, with an intention to compenſate for that material deficiency, that he immediately relates the hiſtory of frequent and ſpontaneous appearances of the Yellow Fever in the continental Spaniſh dominions of North America, and in the iſthmus.

In terminating his preface, the learned author juſtly expatiates on the neceſſity of conſulting and examining all the writings relative to a ſubject of great national importance. That taſk he could not more faithfully diſcharge, than by tranſlating one of the moſt uſeful productions, the account, &c. by *Benja-*

min Ruſh. To him he dedicates an eloquent, but merited tribute of praiſe and reſpect, delineating his candour as an author, his courage in practice, his fortitude during a perſecuting oppoſition to his doctrines and advice, his accuracy of obſervations and perfect knowledge of the ſubject of the preſent inquiry.

Dr. Luzuriaga reſpectfully propoſes alſo to offer his ſtrictures, to ſubſtantiate his arguments, in every point, in which he muſt differ from the American Profeſſor.

2. The catalogue of Portugueſe, Spaniſh, Engliſh, Dutch, North American and French works, which treat of the Yellow Fever, deſerves a particular notice; it compriſes every intereſting paper or eſſay on that ſubject, that were inſerted in any of the European and American periodical collections. They are all mentioned alſo in their own original language, and the whole becomes conſequently a very precious collection for the uſe of the learned; which in time muſt be the means of diffuſing knowledge and ſubſtituting it to prejudice and panick.

3. The notes affixed to the tranſlation are numerous, inſtructive, and well connected with the various points ſubjected to elucidation or controverſy. In reference to the Brownonian theory, which he alleges has been partially and with modification, adopted by Dr. Ruſh, he offers a ſeries of intereſting arguments againſt it. Of the doctrine of the latter on fevers, he ſubjoins an accurate expoſition, offers but few obſervations againſt it, which we do not judge adequate to the merit of the controverted ſubject, nor ſubverſive of the principles upon which it ſtands. Luzuriaga appears averſe to thoſe new modes of thinking, which militate with his favourite, and, I dare to ſay, habitual phyſiology in the manner of Boerhaave, Hoffman and Cullen. How could it be true, that nothing but error and pernicious applications reſult from modern theories?

And ſhould he not impeach his own underſtanding for concluding from the novelty of names, that as they obſcure and confuſe our ideas, they ſhould always be exploded from medical language? To the infatuated Brownonian, we would, like him, repeat the well applied advice of the Poët:

" Parce, puer, ſtimulis; et fortiùs utere loris."

But to the unqualified and excluſive admirer of ancient doctrines, who even rejects the uſeful help of ingenious exertions of the moderns, let us at leaſt recommend to attend to cauſes hitherto unknown:

" Ni refugis, tenuesque piget cognoſcere cauſas."

It is but juſt to cenſure the Spaniſh tranſlator for connecting ſo often, the names of the writers he conſults, with what he ſuppoſes to be their opinions, doctrines, and obſervations, in a ſtyle and propoſitions different from their own, to which he ſeldom takes the trouble to mark any literal reference. There never can be ſufficient reaſon to diſpenſe in controverſy with ſuch a rule, as propoſitions derive always their meaning from antecedents and connexion with their intended deduction. In the notes of Luzuriaga, we find frequently ourſelves in a very ſtrange and complicated dreſs; with difficulty we can make out at laſt our own features. At that rate, the honour of being noticed is very equivocal. How far the author has expoſed himſelf to conſiderable miſtatements, by his own mode of quotation, it would be difficult to aſcertain; but while we reject the deſign or ſuppoſition of unfair inquiries, we regret to be able to point out ſome material inaccuracies of the kind; moreover, ſo many quotations do not conſtitute preciſely a meritorious appearance of learning or erudition; they do not look better indeed than many files and ſtrings of notes, for various ſubjects, more ſymmetrically than judiciouſly arranged.

4. The tranſlation is very correct, and as far as we could judge conformable to the original text. Some pages however have been ſuppreſſed and *condenſed* in a ſhort diſcourſe, expreſſing cenſorious obſervations, on account of Dr. Ruſh declaring his wiſhes that the public could be always informed of the nature and method of cure of peſtilential diſeaſes: this is thought inconſiſtent with his having warned his readers in another place, and appriſed them that his ſucceſsful mode of treatment in 1794, could not be applied in any other recurrence of the epidemic. But if experienced practitioners are aware of whatever alterations of ſymptoms and change of remedy another peſtilential prevalence might conſtitute, what danger can ariſe from their timely and earneſtly recommending practical means, which are to be depended on, eſpecially during calamitous periods of deſertion and death, when the ſick are frequently left diſtreſſed and helpleſs?

Luzuriaga undertakes to ſubſtitute a diſcourſe on Prognoſtics, to the *account* of Dr. Ruſh; becauſe this part has been, he thinks, treated ſlightly and remains very deficient. Another file of medical documents of diverſe authorities, is preſſed on to ſtate all the varieties of prognoſtics on the yellow fever.

Here we muſt ſay that the Spaniſh writer indecorouſly ſuppoſes, that ſuch uſeleſs details have not been known, nor timely thought of; no doubt they were, and were properly alſo, judged uſeleſs. In fact, from the immenſe variety of accidents and ſymptoms known to him, what could he deviſe towards a uniform ſyſtem of noſological prognoſtics? If the taſk of obſervers and practitioners was juſt ſet on foot, to commence an inveſtigation of the characters and effects of this diſeaſe; a minute inquiry of all that could have been written, might perhaps better ſubſtantiate the form of its ſymptoms and prognoſtics. But to thoſe who write to prove, that notions heretofore ſup-

ported, are imperfect and erroneous, a new diſplay of ſayings and quotations on the ſame ſubject, is faſtidious and uſeleſs.

In this article we notice with pleaſure, that collecting the beſt meteorological tables and obſervations, which were obtained at the period of 1793, Luzuriaga takes the opportunity of evincing the fallacy of the inductions made by Mr. Matthew Carey, againſt the local and domeſtic origin of the Yellow Fever, on the ground that atmoſpheric influence of rains and cold rather increaſed the mortality, for he juſtly refers the invaſion of all thoſe fatal caſes to previous more warm and inſalubrious days, &c.

In the laſt part of this work, we are really gratified by a credible, and authoritative diſplay of facts in favour of a good remedy and preventive in the Yellow Fever. "Spain itſelf can offer to enlightened Europe, the greater maſs of thoſe inſtances in which a timely uſe of oil, applied externally or internally, has produced a radical cure." To thoſe reſpectable teſtimonies, if we add thoſe that were of late years collected in the Levant, during the ravages of the plague; we are, no doubt, obliged to give it our examination or aſſent. Ancient documents are not wanted to exemplify the ſalutary effects of that remedy, in any indication congenial to peſtilential diſeaſe. It is our opinion, that frictions of oil and *coatings of idio-electric ſubſtances* will ultimately prove neceſſarily uſeful in plague, Yellow Fever, &c.*

The two addreſſes of our worthy Profeſſor, to his fellow citizens, publiſhed in the year 1799, are ſubjoined to this tranſ-

* An intereſting paper on the ſame ſubject, has lately been publiſhed by Wm. Hunter, A. M. of the Aſiatic Society of Calcutta, and Surgeon of the Marine Eſtabliſhment at Bengal. Vide, Edinb. Med. Jour. No. 6.

lation. The firſt leads naturally the Spaniſh contagioniſt to take a particular notice of the different opinion it promulgates of the non-contagiouſneſs of the Yellow Fever; and again, a liſt of quotations is reſorted to, as numerous and weighty as the circumſtance of the caſe requires. But why are they reſorted to, when it is logically intended to refute them all? Is not that mode of reaſoning called *petitio principii!* What can thoſe authorities prove againſt a belief, rendered ſo recommendable by the rare inſtance of a ſelf-gained triumph, over a contrary doctrine, which the ſame man had formerly adopted, and which he now candidly oppoſes.

It is too often the caſe, that books are analyſed or reviewed, with the view of finding fault and cauſe for criticiſm, or encomium. This taſk being accompliſhed, the reader will like or diſlike the book forever, without knowing much about it. I hope that no ſuch irrevocable judgment ſhall reſult from this account of the above work, as neither is intended.

We wiſhed to give a correct idea of a foreign production, which contains much original matter of important national concern; but ſo interſperſed that it would hardly be made the ſubject of a tranſlation, and probably will never be circulated among us. On the other hand, it was neceſſary for the ſake of controverſy, to ſearch among the comments ſo officially and authoritatively circulated in Europe, whether we could find reaſon to ſuppoſe the doctrine invalidated, of the domeſtic origin of the Yellow Fever, which we have conſtantly advocated in America, and we confidently leave it to the impartial reader, to judge and appreciate.

Luzuriaga has, notwithſtanding, well merited of his country, for preſenting it with one of the moſt uſeful books. To him all writers who have added ſomething to the ſtock of general information, are indebted for being honourably and impartially

noticed. As a writer and a tranſlator, he excels by his learning, and by the laborious compilation he has accompliſhed; as an author, it is true, he is aſtoniſhingly paſſive, among ſo many evidences and arguments, which might ſo eaſily ſubvert all his doctrine; his candour, therefore, is the more genuine and recommendable, and we propoſe it as a model altogether, with the reſpectful, dignified and liberal deportment towards all his adverſaries. Inventive geniuſes obtain fame and celebrity; but profound and learned men, are likewiſe entitled to praiſe and admiration, while reſpect and gratitude, are abundantly treaſured up for them all.

An Account of the Effects of copious Blood-letting, in a Caſe of difficult Parturition. By DR. ~~JAMES~~ JOHN SPENCE, *of Dumfries, in Virginia, in a letter to* DR. BENJAMIN RUSH.

MRS. J—— B——, about 34 years of age, of a delicate habit, very irritable nerves, and ſubject occaſionally to attacks of hyſteria, was taken in labour with her firſt child, on Thurſday, 5th June, 1806. On Saturday evening, the 7th, I was deſired to viſit her. I found her very reſtleſs, her pulſe full and ſlow, tongue white, ſkin hot, very thirſty, but her bowels ſufficiently open. The midwife informed me that her pains had been pretty conſtant, but weak; that the waters had been diſcharged about twelve hours, that the head preſented, but made no progreſs on account of the rigidity of the parts.

About 10 o'clock, P. M. I drew twenty-four ounces of blood from her arm, by a large orifice, and although formerly diſpoſed to faint, when bled, there was not the ſmalleſt tendency to it, on the preſent occaſion, and I could not perſuade her to ſit or to ſtand up. After this bleeding, however, ſhe fell aſleep, and on waking, felt refreſhed, and leſs thirſty and feveriſh; but the labour pains were ſtill inconſiderable.

About twenty hours after bleeding her, having examined per vaginam I found the head preſenting cloſely ſurrounded by the os uteri, and neither of the ears could be felt. The external parts were much ſwoln and ſore to the touch.

As ſhe had now been in labour *four days*, had become extremely impatient and apprehenſive, and her relations much alarmed, I again reſolved to have recourſe to copious blood-letting, to induce ſyncope, and thereby relax the parts and accelerate delivery. I ſtated to her huſband the object I had in view; and accordingly at 10 P. M. I had her placed on the knee of an aſſiſtant, and opened a vein in the arm by a large orifice. While the blood was flowing freely, ſhe was raiſed on her feet, and ſupported by two women, and when about *thirty* ounces were drawn, ſhe began to complain of being fainty; ſaid ſhe was ready to ſink, looked very pale, and large drops of ſweat covered her face. Her arm was now tied up, and ſhe was laid on the bed; a few minutes thereafter, her labour pains came on with great force, and ſhe was ſafely delivered an hour after the bleeding.

Obſervations on Parts of BURNS' *Hiſtory of the Gravid Uterus.*

By WM. P. DEWEES, M. D.

THERE are few things leſs perfectly underſtood than the anatomy of the human ovum before the third month of pregnancy. We muſt alſo include in this charge, the condition of the uterus itſelf, and of its appendages, until this period of geſtation: this has ariſen from two circumſtances chiefly; namely, 1ſt. the infrequent occurrence of ſubjects proper for the inveſtigation; and ſecondly, the minuteneſs of ſome of the parts, or their very gradual, or very ſudden change from their original, or unimpregnated ſtate. Hence has ariſen the variety of deſcriptions given us by different authors, who ſaw rather what they wiſhed to ſee, than what abſolutely exiſted. Imagination ought never to ſupply, what alone ſhould be demonſtrated; it ſhould never give its chimeras, for anatomical facts. In matters of mere opinion, ſpeculation is allowable; but if indulged in to ſupply anatomical deficiency, it ſhould be at leaſt premiſed that conjecture is taking the lead, for want of ſufficient obſervation to ſupply the place of phyſical truths.

I have been led to theſe reflections from a work on the gravid uterus* having juſt come into my hands, in which I conceive there is indulged no inconſiderable licenſe of the kind juſt mentioned. This I truly lament, as there is conſiderable induſtry, obſervation, and acumen diſplayed in many parts of it. The errors, as I conceive them to be, I have no doubt will be corrected by the ſubſequent obſervations and reflections of the author himſelf, as they muſt ſooner or later ſtrike him, as circumſtances not ſupported by anatomy, analogy, or reaſon.

* The Anatomy of the Gravid Uterus, &c. by John Burns.

The firſt circumſtance I ſhall notice will be his account of the manner in which the ovum deſcends into the uterus, and the condition of the parts through which it paſſes. He ſays, p. 150, that "Before the embryo paſſes down through the fallopian tube into the uterus, that organ is every where lined with a vaſcular ſubſtance, which is produced by the action of geſtation taking place in the uterus. This, which has received the name of the tunica decidua, conſiſts of two layers, the inner of which is entire, but the outer is perforated at the os uteri and entrance of the tubes. This outer layer enters, for about an inch, within the fallopian tubes, and deſcends down the ſides of the cervix uteri to its mouth terminating in that gluten which ſhuts it up.

"The ovum is likewiſe covered, (p. 151,) with a vaſcular coat, conſiſting of ſhaggy veſſels, ariſing from the chorion."

I would aſk, what proof is there that, the "" outer layer of the decidua enters, for about an inch, within the fallopian tubes?" is there any? is this a fact warranted either by diſſection or analogy? I think it is not. My reaſons for thinking ſo are, firſt, that it never has been demonſtrated; and ſecondly, that it would be injurious. It would be injurious by ſtopping up the mouth of the fallopian tubes, and thus offer an obſtruction to the paſſage of the ovum; for we are not told that theſe orifices are increaſed in ſize during pregnancy, and if they be not increaſed in ſize at this time, it would be in vain, that the anatomiſt ſhould apply his knife to demonſtrate an efflorescence of veſſels for an inch within their cavities, ſince in their natural ſtate they ſcarcely allow a briſtle to paſs. With what chance of ſucceſs, then, can we hope that this will ever be proved by anatomy, more eſpecially as all reaſon is againſt it? It would be idle to urge that, at this period of pregnancy they acquire

more ſize, unleſs it can be demonſtrated; and the more ſo, as it would ſeem they dilated but to be obſtructed. But let us aſk for what purpoſe is the decidua within the tubes; can any uſe be found for it? The author himſelf urges none. May we not ſafely ſay, it would be injurious, and offer a great riſk to the ovum being detained, and developed there, ſince the ovum itſelf is covered with a vaſcular coat "which is to inoſculate with the decidua reflexa after it enters the uterus?" And if it be covered with "a coat conſiſting of ſhaggy veſſels" for the purpoſe juſt mentioned, why ſhould this union not take place with the veſſels of the decidua within the tubes, ſince it is admitted by all the experimental phyſiologiſts that the ovum or ova may remain ſome days in the tube. At leaſt this has obtained in the inferior animals, and if analogy be admiſſible in an inquiry of this kind, we could have no heſitation to conclude, it would remain equally long with the human female; and which no doubt would be time ſufficient to form a connection every way firm enough, to detain it within the tube; the author himſelf ſays, p. 151, "that when the ovum deſcends into the uterus, it does not fall freely into the cavity, but is every where ſurrounded with a vaſcular coat from the uterus. With this coat the veſſels of the chorion unite; and were we, therefore, at this period," (that is, of its deſcent into the uterus) "to examine the ovum, we ſhould find, that its ſhaggy veſſels united, at one part, with the decidua, at the fallopian tubes," (why therefore, not within them) "and, at every other part, with the inner layer, which is puſhed before it."

From this it would appear that, agreeably to the author, the union between the ovum and the uterus, or at leaſt the production of the uterus, takes place very rapidly; if this be admitted, it ſhould alſo be allowed, that a ſimilar union might take place within the tubes, unleſs ſome good reaſon can be given for the

ovum's not inoſculating with the decidua, as readily as with the decidua reflexa; but it does unite as readily with the one as with the other, according to his own doctrine, for he tells us, p. 153, that when the ovum falls into the uterus in a certain direction that it then unites with the inner layer of the decidua, which afterwards becomes decidua reflexa.

The next circumſtance I ſhall notice is the mode in which the ovum is nouriſhed, and deſcends into the uterus. "The embryo" ſays he, p. 152, "is at firſt a ſmall ſpeck, *growing* cloſe to the ſides of its membranes." We would aſk what is preciſely meant by the embryo growing to its membranes? Have they ever been ſeparate? If they have, by what kind of union are they joined? And where does this union take place? The embryo, p. 152, is ſaid moſt likely to derive its nouriſhment from "that portion of the ſhaggy chorion which covers the part of the membranes to which it is attached." It muſt be remembered that the ovum is now ſet free from the ovarium, and conſequently not attached any where until it deſcends into the uterus to form a union with the reflected decidua; but the embryo muſt have ſupport until this junction is formed; from whence does it derive it? From the ſpongy chorion; from whence does the ſpongy chorion derive it? This the author does not tell us. The chorion either has received a ſtock from the ovarium before its departure (which has never been proven), or the extremities of its veſſels muſt have the power of abſorption, a circumſtance wanting proof; and the more eſpecially as we are told this ſhaggy coat is to be the bond of union between it and the decidua reflexa. For we are immediately after told, "when it deſcends into the uterus, the decidua gives an additional covering, and joins its aid to the increaſed demands of the embryo; and the two vaſcular coats form the placenta." From our author's account therefore, I am much at

a loſs to account for the ſupport of the embryo while it is at the mercy of the fallopian tubes, and before it forms a connection with its mother. It is true Mr. Burns, p. 115, ſeems to think he does away every difficulty by ſaying, "it is probable, that, at firſt, the embryo grows by a kind of hydatid life;" but does this explain it? Is it not as difficult to form an idea of hydatid life as any other? If the ſyſtem by which this kind of life (if we may ſo term it) is maintained be leſs complex, is it leſs ſurpriſing, or of leſs difficult ſolution?

I ſhall now advert to another ſpeculation of our author; here, ſome indulgence may be given ſafely to fancy; but it ought to be admitted as a conjecture, and not advanced as a phyſiological truth. He ſays, p. 153, "as that part of the membranes to which the embryo is attached, generally enters the uterus laſt, it follows, if this account of the formation of the placenta be true, (that is, of its being the joint produce of the ſpongy chorion and decidua reflexa,) that the placenta will be formed originally over that part of the uterus, where the tube enters the decidua, at that ſpot joining with the chorion to form it. But in ſome inſtances the caſe is reverſed, and the embryo enters foremoſt, the reſt of the membranes following it. When this happens, then the inner layer of the decidua, which was ſtretched acroſs the orifice of the tubes, and which is afterwards to become the decidua reflexa, will contribute to the formation of the placenta."

The author has indulged here no common ſhare of conjecture, which by the peculiarity of language gives a very impoſing appearance, and which might be miſtaken for a well aſcertained fact. The manner in which the ovum enters the uterus muſt from the very nature of things ever remain in profound obſcurity. We need never hope for this point to be

cleared up, as the minuteneſs of the ovum itſelf; the impoſſibility of ever comparing the manner in which they fall into the uterus; but above all, the improbability that it ſhould be witneſſed in the act of falling, will ever place this matter among the many, which belong to conception, beyond the ken of man.

It is confeſſedly lawful on this part of the ſubject to indulge in hypotheſis; but it is miſleading too far, when a language is employed that would convey the idea of the fact being thoroughly aſcertained; who would but believe, that the mode in which the ovum enters the uterus, was well ſubſtantiated, when he reads, "as that part of the membranes of the ovum to which the embryo is attached, generally enters the uterus laſt;" p. 153, and again in the ſame paragraph; "but in ſome inſtances the caſe is reverſed, and the embryo enters firſt." Who, I ſay, from this language would ſuppoſe, but what the author had frequently witneſſed the fact, or has had ſome very cogent reaſons for believing, that it entered preciſely as he has preſcribed? The language of doubt is not employed; it is given as a fact of which there could be no diſpute. But we heſitate not to ſay, no one has ever had an opportunity to aſcertain this point; therefore all that is ſaid reſpecting the ſituation of the placenta, muſt be received as conjecture.

It has ever been a puzzle to determine why the placenta ſhould not always be attached nearly to the ſame place of the uterus. To do away this difficulty Mr. B. has aſſerted, that, the part of the membranes, to which the ovum is attached, generally enters the uterus laſt, having before premiſed that, the placenta is the joint production of the decidua and chorion, and that their union takes place oppoſite the part to which the fœtus is attached. It was therefore neceſſary to this theory,

to ſuppoſe, "the part of the membranes to which the embryo is attached," ſhould "generally enter laſt." And to account for the placenta being occaſionally placed over or near the mouth of the uterus, it was alſo neceſſary to admit that, "in ſome inſtances the caſe is reverſed."

I have already ſaid, there was no hope of this ever being demonſtrated; let us inquire now, how far it can be ſupported by facts and reaſon. From the premiſes laid down by Mr. B—— it muſt from his own confeſſion follow, "that the placenta (p. 153) will be formed originally over that part of the uterus, where the tube enters the decidua; at that ſpot joining with the chorion to form it." Now if this be admitted as a fact, we ſhould always find the placenta covering one of the tubes, or in other words that it would always be at or near the fundus of the uterus, except in thoſe caſes, where matters are reverſed, or where the embryo enters the uterus firſt. "When this happens," ſays our author, "then the inner layer of the decidua, which was ſtretched acroſs the orifice of the tube, and which is afterwards to become the decidua reflexa, will contribute to the formation of the placenta. In this caſe by the diſtention of the ovum, and the yielding of the decidua reflexa, the placenta will come at laſt to be inſerted over the mouth, or over ſome inferior part of the uterus."

Agreeably to this doctrine, the placenta muſt be found in one of three places, namely, over the openings of the fallopian tubes, or at the inferior part of the uterus. Does this accord with experience? the gentleman himſelf, I am ſure, upon reflection, will ſay no. There is certainly no part of the internal ſurface of the uterus to which the placenta may not be attached; and I believe it will accord with the obſervation of every accoucheur, when I ſay, I do not think any one part of the ſupe-

rior three-fourths of that viſcus, ſeems more eſpecially ſelected for its implantment. Boudeloque* in combating the opinions of Monſ. Levret on the cauſes of the obliquity of the uterus, furniſhes us with many examples of uncertainty, to what part of this organ the placenta adheres. And Monſ. Levret is of opinion, that the obliquity of the uterus happens when ever the placenta is not attached to the centre of the fundus; now if this be true, it will demonſtrate how rarely it is grafted there, ſince almoſt every woman is ſubject to this complaint. But we will not inſiſt on this negative proof of the variety of places to which this maſs may be attached, ſince the obſervation of every day affords abundant proof of it.

But let us examine for a moment the manner in which we ſuppoſe the ovum to paſs into the uterus. The ovum, we ſuppoſe, when detached from the ovarium, muſt be completely globular, and that its membranes poſſeſs a conſiderable degree of firmneſs, that it may not be burſt by the action of the fallopian tube; now if it be completely ſpherical it muſt revolve frequently in its courſe down the tube, as we preſume it is furthered along by that canal repeatedly contracting from behind; if this be true, to what part of the genital ſyſtem ſhall we attach the power, of meaſuring exactly the length of the tube? to determine the preciſe number of revolutions it is to make, that "the part of the membranes to which the embryo is attached may enter the uterus laſt"; to nicely adjuſt its ſituation before it enters the tube, that the caſe may not be reverſed, and the embryo be made to "enter firſt." I ſay to what part ſhall we give this power? without all theſe precautions we ſhall ſee at once that, the ovum muſt enter the uterus promiſcuouſly; this in fact we ſuppoſe, and hence we find its placenta may be

* Midwifery, Vol. I. Art. Obliquity of the Uterus.

attached indifferently to any part of the internal ſurface of this organ.

From a note in p. 153, we are led to ſuppoſe, that Mr. B. does not entertain the ſame opinion of the ſhape and firmneſs of the ovum as is contended for here, ſince he ſays, "The entrance of the ovum may be compared to the delivery of the child, at full time. In both the membranes protrude firſt; at leaſt they always protrude firſt in labour, owing to the bulk of the child; and moſt commonly they protrude firſt from the tube, although, from the ſmallneſs of the embryo this does not invariably happen." I ſhall not remark on the glaring inconſiſtency of the compariſon, but merely ſtate, what ſtrikes me as the opinion of Mr. B. of the form of the ovum. From what he advances in the note juſt quoted, we are led to ſuppoſe, he thinks the paſſage of the ovum a ſpecies of labour, and that the membranes of it are lax or not completely diſtended, ſince they are puſhed before the embryo, like the membranes at full time before the advancing child. Now, if this be admitted, what is to preſerve the tender embryo from the deſtructive embrace of the contracting fallopian tube?

I conceive that Mr. B.'s ideas on this ſubject cannot be admitted, as the moſt trifling compreſſion would be ſufficient to deſtroy the embryo; and agreeably to his own ſtatement the ovum muſt neceſſarily be ſubjected to conſiderable reſtraint, ſince the decidua lines the tubes for an inch, which muſt ſerve very much to diminiſh their capacity; and alſo, ſubjects the ovum to ſufficient preſſure to lengthen and puſh out the membranes before the embryo.

Beſides, we have the authority of moſt of the experimentaliſts on this ſubject, to ſay, that the ova are completely round,

if any dependence is to be put upon their figures of them; thoſe of De Graaf and Cruikſhank are decidedly ſo, and we truſt that reaſon confirms the opinion. It will now irreſiſtibly follow, that Mr. B.'s ſtatement of the manner in which the ovum enters the uterus is chimerical; and that the ovum muſt frequently apply different portions of its ſurface to that of the tube during its paſſage through it, conſequently, no certainty can obtain with reſpect to the part it may preſent to the opening at the uterus.

Philadelphia, 15th November, 1806.

DEAR SIR,

The above paper was written nearly a twelvemonth ſince, but with a different view than for publication; but as I have for the preſent abandoned the object I then had in view, I ſubmit it now to your diſpoſal.

I am, Sir, yours with eſteem,

WILLIAM P. DEWEES.

DR. JOHN REDMAN COXE.

Caſe of Hydrocele, ſucceſsfully treated without a ſurgical Operation. Read before the Philadelphia Medical Lyceum, November 5th, 1806, *by* WILLIAM SHAW, M. D.

ON the 19th of January, 1804, I viſited Mr. A. S. who was afflicted with Hydrocele of the Tunica Vaginalis. The tumour was about four inches in diameter at the lower and moſt depending part, and extended upward, in the direction of the ſpermatic cord, to the abdominal ring. It was of a pyramidal figure, nearly ſix inches in length. The teſticle could not without great difficulty be diſcovered.

The tumor was tranſparent at the lower and anterior part. It gave him very little pain, unleſs when the bandage that ſuſpended it was removed, or when he ſtood near a fire, in which ſituation, he felt a ſenſation of ſcalding: the principal inconvenience was from its ſize. The diſeaſe had been gradually advancing for five months before I was conſulted. I immediately propoſed an operation for the removal of the water; but to this he objected, ſtating that his buſineſs, at that time, would not admit of his confinement. He wiſhed me to make trial of ſome other means. I then commenced the cure by friction to the part with ſtrong mercurial ointment, and by ſuſpending the ſcrotum in a leather bag, on the inner ſurface of which was ſpread a quantity of the ſame ointment. The parts were directed to be well waſhed with warm water and ſoap, once in three days. He was occaſionally bled, and draſtic purges were adminiſtered twice or thrice a week. He proceeded in this courſe for three weeks without any apparent benefit, though the mercury had cauſed conſiderable ſoreneſs of his gums, accompanied by a gentle ptyaliſm. During this period he loſt from the arm near fifty ounces of blood. Diſappointed in the reſult of thoſe meaſures, and my patient ſtill unwilling to undergo a ſurgical operation, I attempted the cure by a different treatment, as ſoon as the mercurial action had ſubſided. Two grains of tartariſed antimony, combined with an equal quantity of powdered digitalis, were given him morning and evening, which vomited pretty briſkly, and kept his bowels open. He alſo was deſired to bathe the parts frequently in the day with cold water. This plan was continued about two weeks, when the whole of the water ſeemed to be removed, and the ſcrotum and teſticle reduced, almoſt to their natural ſize. The medicine was now diſcontinued, and leeches were applied to remove a little inflammation, which ſtill exiſted in the teſticle and ſpermatic veſſels. The cold water was employed a few weeks longer, in order to ſtrengthen the parts, and prevent a return of the diſeaſe.

No ſymptom of the complaint has, however, yet recurred, and he now enjoys good health. It is proper to obſerve, that this man was not confined during the cure; but was able to attend to his buſineſs, which is a circumſtance in favour of this plan.

Some Account of the Appearances on Diſſection in certain Caſes of Yellow Fever, and Notice of ſome Symptoms of the Diſeaſe, as it appeared in the City Hoſpital in the Year 1805. *Read before the Philadelphia Medical Lyceum,* —— 1806, *by* JOSEPH PARRISH, M. D.

AS the ſubject of Yellow Fever has become very intereſting to the citizens of the United States, and as in ſeveral of our populous cities, many thouſands have fallen victims to its ravages, it becomes the duty of thoſe practitioners of medicine, who have had an opportunity of obſerving many caſes of this formidable diſeaſe, to render all the information in their power reſpecting it; and although the limits of the preſent eſſay will not admit of entering at large upon the ſubject, yet if it goes no further than to confirm ſome facts related by others, I ſhall have at leaſt, made an effort to contribute my part; and ſhall proceed to an account of certain appearances on diſſection, and to notice ſome ſymptoms of the diſeaſe, as it appeared at the City Hoſpital in the ſummer and autumn of laſt year.

In concert with my friend and colleague, (the late) Dr. John Church, and aſſiſted by Dr. S. Tucker, and our preſent ſecre-

tary, J. Perkin, who both reſided with me at the hoſpital, I examined a conſiderable number of the patients who died at that inſtitution.

The uniformity of diſeaſed appearances in the ſtomach was very remarkable, and it was obſerved that where the inflammation of this viſcus ſeemed to have been moſt violent, the quantity of matter denominated black vomit was the leaſt; and in ſome inſtances where the whole ſurface of the villous coat had been very highly inflamed, there was a total abſence of black matter.

The liver in ſome ſubjects was natural, in others it was tinged of a yellow colour; the gall-bladder contained bile which at firſt ſight appeared perfectly black, but on dipping linen or white paper into it, a dark green colour was immediately evident, except in two inſtances where it was of a dark orange colour and thick conſiſtence; it reſembled the ſyrup of rhubarb. The ſtomach in both inſtances contained the black vomit. When this fluid was ſubjected to the ſame teſt as the bile, it was generally found of a dark reddiſh or chocolate colour, but never had a greeniſh caſt.

Two ſubjects were examined who died comatoſe; the brain in both inſtances, and the membranes inveſting it, appeared natural: in one of theſe caſes, the ſtomach contained black matter, and an introſuſceptio of the ſmall inteſtines was diſcovered in two portions; in the other, the ſtomach did not contain any black matter, and had been but very ſlightly inflamed; the veſica urinaria had been in a ſtate of great inflammation, it had a number of blotches upon its inner coat of a deep ſcarlet colour. A part of the ſmall inteſtines was of a very dark colour approaching to gangrene, its inner coat was lined with a bloody mucus. A ſimilar appearance was obſerved in ſeveral other

ſubjects.* Two other caſes of introſuſceptio were alſo met with. Worms in the inteſtines were frequently obſerved during the courſe of the epidemic; I have known them to crawl from the patients during their illneſs.

Three caſes occurred wherein there was no ſecretion of urine, all of them ſweated moſt profuſely, and all died. One of them was examined after death; the bladder did not contain a tea-ſpoonful of urine. This patient was affected with a paralyſis of his lower extremities a few hours before his deceaſe; his mind appeared to be perfectly collected at the time, and he deſcribed his ſenſations with great clearneſs.

The preceding appearances induced me to concur with my colleague and others, who aſſert, that the contents of the gall-bladder and the black vomit are ſpecifically different; the latter fluid being blood diſcharged from the capillary veſſels of the villous coat of the ſtomach; in ſupport of this doctrine I will offer a few obſervations.

1ſt. When a part is inflamed, the blood-veſſels of that part will become enlarged as was proven by John Hunter, in his experiments on the ears of rabbits; again, during this ſtate of inflammation, the veſſels deſtined to carry ſerum, admit red blood; we have repeated proofs of this in ophthalmia.

2d. The ſtomach is generally affected very early and potently in the diſeaſe; as a proof of it, we need go no further, than to obſerve the extreme tenderneſs of this organ, evinced by ſlight preſſure on the epigaſtric region.

3d. In the laſt ſtage of the diſeaſe we frequently ſee dark diſſolved blood, oozing from bliſtered ſurfaces, from the gums and tongue.

* In one caſe the ſmall inteſtines contained a quantity of pus.

From theſe circumſtances I infer, that as the ſtomach is early affected by inflammation, its minute veſſels become enlarged and admit blood, and as we ſee this fluid in a diſſolved ſtate diſcharging from the parts already enumerated, it is eaſy to conceive, that the ſame thing ſhould occur from the veſſels of the ſtomach, eſpecially as in a large majority of inſtances, the diſeaſe has been forcibly determined to this particular part.

Several caſes occurred where the ſtomach appeared to have been highly inflamed, and there was a total abſence of black matter. Is it not reaſonable to conclude that in theſe patients the inflammation muſt have been leſs early in its commencement, or ſlower in its progreſs, or elſe the diſeaſe has been ſo unuſually violent, that death has terminated the conflict, while the capillary veſſels were in a ſtate of activity inſtead of diſorganization? In ſupport of theſe conjectures, it may be ſtated, that the moſt highly inflamed ſtomach I ever met with, was in a young man, who died within 48 hours from the period of his attack.

The diſcharge of black matter from the ſtomach, is often preceded by a ſymptom, which ſtrengthens the opinion which I am endeavouring to ſupport. Who has been converſant with the diſeaſe, that has not witneſſed the *pauſe*, the *awful pauſe* between the violent ſymptoms of the firſt ſtage, and that wherein the ejection of black matter gives an intimation that death is at hand. During this truce as it were from its horrors, the patient will lie quite compoſed, pulſe ſlow and ſinking, countenance marked with an indeſcribable aſpect, the patient and his friends flatter themſelves that the diſeaſe is ſubſiding, perhaps the only complaint is a ſenſe of weight or heavineſs about the ſtomach, but a few hours convinces them that the black vomit, that "harbinger of death is at hand." Now is it not probable that this ſtate is induced by the ceſſation of inflamma-

tory action in the blood-veſſels of the ſtomach, and that at the moment of apparent tranquillity theſe veſſels are gradually pouring their contents into that organ; when a ſufficient quantity is accumulated, its action is excited, and the matter conſtituting black vomit is diſcharged.

May not the colour depend on two circumſtances; in the firſt place, the blood diſcharged during hemorrhages, in the laſt ſtage, is always dark, and when thrown into the ſtomach, it is not in a ſituation where it would receive any conſiderable portion of oxygen, and even if this were the caſe, I would aſk whether its being in a diſſolved ſtate would not leſſen, if not entirely deſtroy its affinity for that gas.

That blood effuſed into a cavity will aſſume an appearance which bears a ſtrong reſemblance to the fluid in queſtion, is inferred from the following fact which came within my own obſervation.

During a term of attendance on the Philadelphia Diſpenſary laſt ſpring, a female patient in Gaſkill-ſtreet, came under my care, with pneumonia. As the ſymptoms were urgent, the lancet was employed with freedom; bliſters were applied over the part in pain, and the uſual antiphlogiſtic mode of treatment was adopted; but on the evening of the fifth day, her pulſe flagged, reſpiration became laborious, about one pound of blood was thrown up through the trachea during the night, and finally ſhe died. On opening the thorax 18 hours after death, in the preſence of Henry Wiſmer, at that time apothecary to the inſtitution, the right ſide was found to contain a large quantity of black-coloured offenſive fluid, I think at leaſt two quarts; ſeveral clots of coagulated blood were adhering to the ſide of the thorax, and it appeared as if the fluid was derived from the rupture of a pulmonary veſſel, which diſcharged a portion of its blood through the trachea, while a much larger

quantity was effuſed between the lung and the parietes of the thorax. I preſerved about ſix ounces of it, and now preſent it for the inſpection of my medical brethren, who have often ſeen the black vomit.

Previouſly to cloſing the ſubject, permit me to obſerve, that as I have ſeen ſome remarkable caſes of recovery after the brain has been apparently much affected by the diſeaſe, but none after black vomit, (except one inſtance of a female child) ſo in an urgent caſe where danger appeared to be very great, my hopes of the patient's recovery would be ſtronger if the force of the diſeaſe was exerted even on the tender brain itſelf, inſtead of the ſtomach.

A deficient ſecretion of urine, accompanied with profuſe ſpontaneous ſweats, affords a prognoſis of the moſt unfavourable kind, although a tranſient obſerver might readily conclude that the ſweating would effect a ſolution of the diſeaſe; that this ſymptom is the conſequence of paralyſis in the ſecretory veſſels of the kidney, ſeems to be confirmed by the fact, that in one of the caſes, the lower extremities were ſimilarly affected.

The natural appearance of the brain, where coma had been the prevailing ſymptom, affords room for ſome ſpeculation. How far it may be affected by ſympathy with the ſtomach I ſhall not pretend to determine; in both caſes the ſtomach was diſeaſed, although it muſt be admitted, that in one of the ſubjects the inflammation of this viſcus appeared to have been unuſually ſlight.